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Mediationism and the Obfuscation of Memory

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Memory theorizing is going nowhere. The reason is that it is rooted in mediationism, the doctrine that memory is mediated by some sort of memory trace. Mediationism is the basic tenet of those who seek the substrate of memory; for students of memory per se it is merely a metaphor, and moreover an unfruitful one, for it cannot be penetrated by the methods of psychology. The rejection of mediationism would serve both to replace mechanistic theories with laws or other modes of explanation and to focus research on the actual experience of memory and on the context in which it occurs. The ensuing advantages are discussed and illustrated.

Memory theorizing appears to be progressing nicely. Indeed, judging from the sheer number of pages it consumes in the literature, it appears to be progressing as never before. But the appearance is deceptive, for something is wrong. Very wrong.

The argument to be made here is that our memory theories are not only failing to generate a lasting understanding, but are actually counterproductive. The problem is a fundamental one, and its correction will require a radical change in the way memory is conceptualized. Before pinpointing the problem, let us review the contemporary scene.

An Era of Cheap Theories

Some years ago, Benton Underwood (1972) observed, "memories now have attributes, organization, and structure; there are addresses, readout rules, and holding mechanisms . . . our memories are filled with T-stacks, implicit associational responses, natural-language mediators, images, multiple traces, tags, kernel sentences, markers, relational rules, verbal loops, and onebuns" (p. 1). The ensuing years have been no less bountiful, and today's memories are burdened as never before.

Such a lavish array of hypothetical constructs offers a boundless source of theories. And researchers—who themselves abound in unprecedented numbers—eagerly select from among these constructs and assemble them like so many pieces of a child's building kit to obtain their very own theories. Once formulated, a theory has several safeguards. First, it is unlikely to draw much criticism from other researchers, for other researchers are primarily concerned with their own theories. Second, when a theory does attract criticism, the critic almost always turns out to have misunderstood, and the theory stands as originally proposed. Third, on the rare occasion a criticism demands action, fine tuning will almost always suffice. Thus, the chances of a theory having to be abandoned or even appreciably revised as a consequence of criticism are vanishingly small, and hence researchers can be confident that their theories will stay alive just as long as they continue to nourish them.

So it is, then, that we have entered an age of personalized theorizing. But

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is this proper? Are theories really supposed to proliferate the way they have? Do a myriad of private concerns constitute the fundamentally public enterprise that is science?

In pondering these questions, we need to consider the reason for all the theorizing, to enquire what lies behind it. We can begin by dismissing any suggestion that it is the result of a deliberate policy laid down by those in our field who wield the most power. To be sure, journal editors typically promote superficial explanations and personalized theorizing by insisting that the findings of a research report be brought to theoretical account. But they are not breaking faith with those they serve, for virtually everyone involved in memory research is skeptical of the scientific mettle of anyone content with investigating someone else's theory without at least modifying it sufficiently to justify proclaiming their own version.

Could the proliferation of theories have its origin in faulty communication? Effective communication is a sine qua non of science, and memory psychologists typically do poorly by this criterion. On the other hand, their failures are not for lack of trying. Indeed, they struggle mightily for clarity, frequently devoting half the pages of their research reports to the effort. To argue that more careful description of memory theories would facilitate their refutation and so help keep their numbers in check would not, therefore, help matters much.

Stemming the flood of memory theories will, of course, require our being less prodigal in using hypothetical constructs. But resolving the root problem with our theorizing will require something more. Imagine, in a flight of fancy, that a wand were waved and some of the things we have stuffed into the rememberer magically disappeared. If the proportion of things thus done away with were large enough, the number of theories would be reduced below the number of researchers, and theory development and evaluation would become more of a shared endeavor. But it would not follow that we would be able to put the theories that remain to decisive tests.

The proliferation of hypothetical constructs, the problems researchers have in unambiguously describing their theories, and above all the difficulty in devising critical tests to discriminate among theories are to a large extent symptoms of a deeper problem, one that reaches down to the level of pretheoretical orientation. In a word, the problem is mediationism.

Mediationism

I am using the term *mediationism* to refer to the doctrine that remembering an event requires that a representation of that event be embodied in a "memory trace" that is retained over the time between the event's occurrence and its recollection. Memory trace sometimes goes by other names, such as engram and representation, and it may be considered to be permanent or to fade or otherwise change over time. But, as it has since at least as far back as classical times, the core idea remains the same: As long as an event from the past can be remembered or can otherwise affect us, some sort of trace of that event is being retained and forms the basis of the memory or other effect.

Mediationist theories are often concerned, not just with the storage of an event in a memory trace, but with the retention of the trace within a store. As has been documented by Roediger (1979), this store within the rememberer has been characterized in a bewildering number of ways—from storehouse (Locke, 1690/1850) to garbage cans (Landauer, 1975) to belly (Augustine, 399/1943; Hintzman, 1974)—although apparently on no other basis than whim. For present purposes, not only are the differences among these characterizations unimportant, but so is the distinction between the idea of store in whatever guise and the idea of memory trace. Indeed, our concern is with the com-

mon function of the store and trace, namely that of storing information over time. Whether their emphasis is on the trace or the store or both, mediationist theories are grounded in the idea that memory involves three distinct stages: an encoding stage, in which information is registered in the rememberer; a retention stage, in which that information is maintained in the rememberer, however imperfectly, over time; and a retrieval stage, in which the information somehow helps effect recollection or otherwise influences behavior.

The doctrine of mediationism has extraordinary appeal, one that relatively few students of memory have resisted. One student of memory has gone so far as to say, "to my knowledge it has never been seriously argued that an event can effect another over a temporal gap" (Rosen, 1975). Some exceptions can, nowadays at least, be found. In particular, the philosopher Norman Malcolm (1977) has rejected mediationism in an especially thorough analysis. Among psychologists, Kolers (1973; see also Kolers & Roediger, 1984; Kolers & Smythe, 1984) and some who favor an ecological perspective (e.g., Bransford, McCarrell, Franks, & Nitsch, 1977; Gibson, 1966, 1979) have deliberately avoided, or even rejected, the mediationist conception of memory—or at least of memory in some senses of the term. Others (e.g., Neisser, 1967; Rubin, 1988) are clearly wary of mediationism and, even if not always with complete success, have struggled to rid themselves of it. For example, Neisser has argued that as replicas of what they represent memory traces are no more faithful than are dinosaur bone chips. But not everyone is so circumspect. Indeed, an overwhelming majority of memory theorists are unabashed mediationists.

The Flaw in Mediationism

I believe that the sorry state of memory theorizing is a direct result of adopting the mediationist doctrine. Were we to disregard the doctrine, we would be less prone to indulge in personal theorizing, we would rid ourselves of the essential cause of our communication difficulties, and we would clear the clutter that more than anything else stands in the way of a better understanding of the nature of memory.

What is the basis for these assertions? Why do I single out mediationism as causing so much trouble? The essential problem with mediationism and the reason for the theory quagmire it has created is that its three stages constitute a level of complexity beyond the analytic power of experimental psychology.

Stripped to its bare essentials, the memory experiment consists of doing something to the subject and determining its effect on later behavior. As I have argued before (Watkins, 1981), this procedure is inadequate for discriminating between alternative explanations that may be cast within the three-stage mediationist framework. To illustrate the point, consider a hypothetical experiment in which subjects are randomly assigned to two groups. Each group is given a list of words to study, and the only difference in the treatment of the two groups is that for one the stimulus words are words that occur frequently in everyday usage and for the other they are words that occur only rarely. Suppose, as well we might (Hall, 1954), that in a subsequent test the first group recalls significantly more words than does the second group. How should this finding be fitted into the three-stage framework?

The problem is not that this is difficult to do, but rather that it can be done in more than one way. How frequently a word occurs in everyday usage could affect, say, the richness of its encoding, perhaps by way of the number of other words it calls to mind or its likelihood of conjuring up a mental image. But a word's frequency of usage could also affect how its trace fares during the retention stage. For example, the trace might be strengthened whenever the rememberer thinks about

the word or perhaps about a related word, and the likelihood of such thoughts would presumably be greater for subjects in the high frequency group. Finally, the more frequently a word is used, the easier it could be to locate its representation in permanent storage at the time of test, in which case frequency of usage would have its effect at the retrieval stage. The important point is that there is no way, even in principle, of distinguishing between these alternatives. One interpretation might be said to be more elegant than the other two, but there is no agreed upon rule or set of rules for deciding which is right and which are wrong.

In what might be seen as an attempt to sharpen the issue, Tulving and Pearlstone (1966) drew a distinction between availability and accessibility. They characterized (a) events that are not being retained in memory (whether because they were never registered or because they were registered but have since faded away) as being unavailable, (b) events that are being retained but are not retrievable under the prevailing conditions as being available but not accessible, and (c) retrievable events as being both available and accessible. Had Tulving and Pearlstone delivered on these distinctions and operationalized the concept of availability, then my charge that a mediationist conception of memory is fundamentally flawed would be seriously undermined.

What Tulving and Pearlstone (1966) did succeed in doing was to underscore the possibility that an event that cannot be recollected under one set of conditions might be recollected under another—a fact that, under the tradition of associationism, had received comparatively little attention. This they did by showing that a previously studied word that is not remembered under conditions of free recall might well be remembered in the context of a related word. What they did not succeed in doing, however, was to operationalize availability. In particular, their claim of having demonstrated availability without accessibility—its wide acceptance notwithstanding—was spurious. Thus, what they took as evidence that an event can be available but not accessible was, to use their terms, nothing more than evidence that an event can be accessible under one set of conditions but not under another. To be sure, within their framework accessibility logically implies availability, but availability is a superfluous concept. The root of the problem is the lack of a procedure for establishing unavailability. How can we ever be sure that a memory failure is due to the relevant information being unavailable and hence inaccessible under all conceivable conditions rather than being merely inaccessible under the prevailing conditions?

It was once common, and even now is not uncommon, for researchers to assume that the answer to this question could be obtained with a recognition test: If an item is not recognized, then it is simply not available in memory. Other researchers—Tulving, interestingly enough, prominent amongst them—have rejected this assumption. They have done so by demonstrating the fallibility of recognition with programs of research showing that a subject may fail to recognize an item even though he or she may recognize it in another context (Thomson, 1972; Tulving & Thomson, 1971) or even produce it in response to a context cue (Tulving & Thomson, 1973). Similarly, research has shown that the perception (Jacoby & Dallas, 1981) or the production (Segal, 1966) or the learning (Nelson, 1978) of items is enhanced when the items are presented beforehand, even if they are not recognized as having been presented beforehand.

It has been countered that such findings do not in principle demonstrate the fallibility of recognition as a test of availability and that they do so in practice only because of a peculiarity of the memory items used in this research, namely individual words (Anderson & Bower, 1974; Martin, 1975). The ar-

gument is that words typically have more than one meaning and that it is a specific meaning rather than the word itself that is retained. Thus, if it is assumed that a word may be assigned one meaning upon its original presentation and a different meaning upon its re-presentation as an item in a subsequent recognition test, recognition failure of a demonstrably available word can be explained without relinquishing the assumption that recognition is in principle the ultimate test of availability. This argument has been critiqued in detail elsewhere (Watkins & Gardiner, 1979). It was found wanting for a variety of reasons, including the finding of context effects in recognition memory of items (unfamiliar faces) that cannot be plausibly assumed to have multiple meanings (Watkins, Ho, & Tulving, 1976; Winograd & Rivers-Bulkeley, 1977).

So it is, then, that psychology cannot distinguish unavailability from inaccessibility. The concept of availability, and with it the whole mediationist framework, is beyond the reach of the psychologist's techniques. The result is that, although mediationism is a veritable cornucopia of memory theories, they are all sham theories.

Can Mediationism Be Discarded?

We have identified the fundamental flaw in mediationism as a framework for memory theory and must now consider the implications of abandoning this framework. What does it mean to conceptualize memory without recourse to the idea of some sort of memory trace?

We need at this point to distinguish between the validity of the mediation assumption and the case for adopting it in our theories. As critical as it is, this distinction is hardly ever made. It would be hard to deny that remembering implies an enduring change in the rememberer or that the quest to learn more about the substrate constitutes a legitimate—even exciting—science. But such a science is not the same as the science of memory. Students of the substrate of memory, unlike students of memory, have or seek techniques for studying the retention stage independently of the other stages. Students of memory do not have such techniques and never will have. The distinction is critical.

Students of memory are too quick to deny the possibility of rejecting mediationism without resorting to magic, too quick to deny that we can have a science of memory without postulating something to bridge the gap between the time of an event and the time at which the event is recollected or otherwise affects behavior. They are, in effect, dismissing as fatuous the idea of action at a distance, and they might do well to reflect on cases in which the idea has proven useful. One such case is Newton's law of universal gravitation, which says that two bodies attract one another with a force proportional to the product of their masses divided by the square of the distance between them. The formulation of this law was without question a significant scientific development, yet it did not resort to tethers. My point here is that just as physicists can study in a meaningful way the effect that a body at one place has on another body at a different place without invoking a physical substrate to bridge their spatial separation, so psychologists can study in just as meaningful a way the effect that an experience at one point in time can have on experience or behavior at another point in time without invoking a physical substrate to bridge their temporal separation.

The viability of such an approach is demonstrated by our own example. For the past decade or so we in our laboratory have conceived, conducted, and reported memory research without reference to any physical connection between past and present. Instead, we have opted for a form of functionalism, a perspective from which memory is regarded as a function of (a) the rememberer's physical environment and state of mind at the time of test, and

(b) the rememberer's history. More will be said of this research shortly, but the important point is that its very existence demonstrates that a mediation framework is not necessary for memory research.

Theorizing Without Mediationism

Giving up mediationism would not mean giving up theorizing. It would mean changing the nature of theorizing, however, for by definition it would mean doing away with the memory trace. The memory trace is but one of a countless number of hypothetical constructs within the mediationist's arsenal, but it is the heart of mediationism and the essential reason why today's theories are inherently more complex than the phenomena they purport to illuminate. Moreover, the trace plays host to all manner of unconscious activities, and cutting it out of our thinking would, directly or indirectly, put an end to an entire parasitic netherworld.

What kind of theorizing would mediationism give way to? Without traces and their attendant structures and processes, what issues would form the focus of memory theorizing? And how, without the mechanism afforded by mediationism, would theorists explain anything? These questions are not easy to answer. One reason they are not easy to answer is that a nonmediationist, or "direct memory," perspective is more a way of not thinking than a way of thinking. Another is that direct memory theorizing does not have a strong constituency to which we might refer. Given these limitations, I will constrain my comments to a few speculations based on those characteristics of our own theorizing that have been fomented by our own rejection of mediationism.

With regard to what the emphasis of direct memory theorizing might be, it seems reasonable to suppose that, by ceasing to look inward to the trace, theorists would be more likely to look outward to the context in which memory occurs. In other words, direct memory theorists would, roughly speaking, do for memory what Gibson (1950, 1966) and his followers have done for perception. One likely effect would be a strengthening of the relation between basic and applied research. Another would be the development of general abstract schemes for describing the outside world from a memory perspective, schemes that could be used in identifying, weighing, and deciphering the relation among the key variables operating at the time of an event's occurrence and at the time of its recollection. An indication of our own thoughts on some aspects of this matter can be found elsewhere (Watkins, 1979, 1987; Watkins & Gardiner, 1982).

To bring out the role of the stimulus environment in determining memory would by no means be to deny a role for the rememberer. Indeed, the rejection of mediationism could well set off a concerted effort to unravel the roles of the rememberer and the environment in shaping the remembering process (see Watkins, 1989). Today's theories fail to promote such an endeavor because they recast stimulus control as a set of hypothetical processes within some hidden part of the rememberer's mind. Stimulus control has, of course, long been of central concern to the behaviorists, but if mediationists have failed to do justice to the stimulus world the rememberer inhabits, behaviorists have ignored as a matter of policy the role of the rememberer's own willful control. A more balanced perspective is sorely needed. Memory serves a biological function, and to understand it adequately requires consideration of both the needs of the rememberer and the nature of the real world in which the rememberer evolved (cf. Baddeley, 1988; Jenkins, 1979; Neisser, 1978).

Whether explanation is possible without recourse to the mechanism allowed by mediationism is a question that turns on the meaning of explanation. It has become clear to me from

reviews of manuscripts and countless informal discussions that, as a rule, recollection is considered adequately explained only if it is traced back through an unbroken chain of hypothetical structures or processes to the occurrence of whatever is being recollected. This viewpoint is unnecessarily restrictive (see Braithwaite, 1953; Hempel & Oppenheim, 1948). The "why" of a phenomenon can often be satisfactorily apprehended merely by relating the phenomenon to temporally remote antecedent conditions. "Because there was a full moon last night" would satisfactorily answer the question "Why are you late?" if the questioner knew that the person addressed was in the habit of being late the day after each full moon but never knew or had forgotten that the moon was full the previous night. Of course, one might seek an explanation of why a law is as it is, of why someone would be late after a full moon. But even if neither the person concerned nor anyone else had the slightest idea, the regularity in behavior would be sufficient to account for this particular lateness as an instance of a pattern of lateness, and in at least this sense the particular lateness could be said to be explained.

The utility of empirical laws can be illustrated with one we refer to as the cue overload principle (Watkins, 1979). It states that recall is mediated by memory cues and that these cues are subject to overload. Thus, a cue loses its effectiveness in triggering memory for any given event or item of information as the number of other events or items it comes to subsume is increased.

The concept of cue overload has, over the decades, been invoked in diverse guises to account for a variety of phenomena. Generally speaking, however, it has been applied on an ad hoc basis, and usually in theoretically charged terms that obscure its generality. Our own preference is to strip the concept down to its simplest terms, terms that can stand as a far-reaching law. Perhaps the most obvious application of this law, or cue overload principle, is to the list-length effect, which it explains with no other assumption than that recall is, at least in some degree, mediated by some sort of "list" cue. As list length increases, the list cue comes to subsume more and more items and so loses its effectiveness for any given item, and hence the probability of that item's being recalled declines. The list-length effect is lessened when the list items are categorized, whether as a result of the experimenter's having deliberately selected each item to conform to one of several obvious categories (Bower, Clark, Lesgold, & Winzenz, 1969) or of each individual subject's having covertly grouped the items of a nominally uncategorized list on a subjective basis (Tulving, 1962). The cue overload principle accounts for this lessening of the list-length effect with the plausible assumption that recall is, in part, mediated by cues corresponding to the categories, for the load on the list cue would then be reduced from the number of items to the number of categories. Similarly, as I have detailed elsewhere (Watkins, 1979), a cue overload explanation can be given for such phenomena as proactive and retroactive interference (see Postman, 1971), the buildup and release from proactive interference (Wickens, Born, & Allen, 1963), the beneficial effect of extralist cues (Bahrick, 1969), and the inhibitory effect of part-set cuing (Slamecka, 1968).

Thus, the cue overload principle demonstrates the power of a simple, empirical law in providing a meaningful account of seemingly diverse phenomena. It shows that the concept of action at a distance can be usefully applied to the study of memory, and that explanation does not necessitate recourse to mediationism.

Research Without Mediationism

Our discussion of the consequences of rejecting mediationism needs now to be extended beyond the realm of the-

orizing to that of empirical enquiry. We need to ask how the rejection of mediationism would affect the conduct of research. Again I can only speculate, and again I will do so on the basis of idiosyncrasies of our own research. It does seem likely, however, that one general consequence would stem from a simplification of theorizing. Mediationism fosters complex theorizing, which in turn fosters complex research questions, questions that more often than not take the form of higher order interactions. The result is a great waste of research effort. The force of this point can be illustrated with a personal parable. A few years ago, the Psychology Department at Rice University was in need of furniture, and for a time I kept a vigil for pieces discarded by other departments. One day I chanced upon such a piece. It was handsome and well constructed, but at the same time it was complex—a sort of table but with a two-level top and a rather odd shape. Clearly, it had been made to meet a particular need, one that presumably no longer existed. Notwithstanding all the time and expertise that had gone into its making, the singular nature of this item of furniture rendered it of no use even to those whose needs were great, and it was indeed thrown away. In the same way, research designed to address some person's individual theory is unlikely to be of any use once that person allows the theory to wither and die. Small wonder that, as others (e.g., Newell, 1973) have lamented, the study of memory has not been an especially cumulative enterprise.

Were we to shed mediationism, theorizing would in all likelihood become less labyrinthian, and research questions more straightforward. Counterbalancing requirements might still complicate the details of an experiment but, compared with those found in current issues of our research journals, the essential findings would be simpler. And simple findings, like simple items of furniture, can be used and reused. They would form a cumulative body of knowledge, and so would free future generations of researchers from the need to start anew.

Aside from its comparative simplicity, postmediationist research would reflect postmediationist theorizing. Assuming my conjecture of more attention to the context of memory was borne out, we could expect to see an intensive study of stimulus control over the remembering process, and of how external stimuli interact with willful control in determining memory. We could also expect to see an extensive program of research investigating proposed schemes for categorizing the stimulus environment, the relative potencies of various classes of memory cues, and the contingency relation between such classes.

A method for determining the contingency relation between classes of cues that captures the spirit of this conjectured research program has been proposed by Tulving and Watkins (1975). In essence, it involves estimating for a given population of target items (a) the proportion that can be recalled in response to cues of one class, (b) the proportion that can be recalled in response to cues of another class, and (c) the proportion recallable to the cues of one of these classes but not to those of the other. When precautions are taken to minimize the chances of their being influenced by prior cuing, these three proportions are sufficient to derive a contingency table for the two classes of cues. Contrary to the terminology of the original Tulving and Watkins article, I like to think of such contingency tables as "cuegrams," or descriptions merely of the relations between classes of cues, rather than of the nature of hypothetical memory traces (Watkins, 1979; Watkins & Todres, 1978). From our theoretical perspective, changes of memory—whether the result of the passage of time, intervening events, or something else—are nothing more nor less than changes in cuegrams.

In addition to a stepped up consideration of the external context of mem-

ory, the rejection of mediationism would in all likelihood result in more careful attention to the experiential side of memory. Important as it is, this point can be overlooked. In our own case, accusations of "rank Skinnerianism" have been common. It may be true that ours is a radically functional approach to the study of memory and that it contains more than a grain of behaviorism. But Skinner and his followers have no place for mental life, whereas we consider memory to be, of its very essence, a mental phenomenon.

This fact is reflected in our research, for the subject matter of a substantial proportion of our studies is grounded in subjective experience. Many of these studies have dealt with echoic memory and other facets of that quintessentially experiential phenomenon William James (1890) called primary memory,[1] and others have dealt with more willful, or strategic, processes such as rehearsal. It is of interest to note that our rehearsal studies have led us to conclude that effective rehearsal is much less pervasive than most theorists believe (see Watkins & Peynircioglu, 1982), and we have difficulty in suppressing a suspicion that mediationists fail to introspect as carefully as they might. Such a failure would not be entirely surprising, for the mediationist framework allows ready substitution of hypothetical unconscious processes for conscious processes, and hence the reality of any conscious processes that are invoked tends not to be critical.

Although most of our investigations of the phenomenology of remembering an event have focused on the time at which the event occurred, we are also very much interested in the remember-er's experience at the time at which the event is recalled. Of particular interest to us is the idea, originally based on introspection and since confirmed with objective tests (Watkins & Kerkar, 1985), that recall is a good deal more generic than is usually assumed. The objective confirmation involved a wordlist experiment in which some words were presented once and others twice. All presentations were tagged with an identifying attribute so that we could gauge the relative recallability of the individual presentations. In particular, we used attribute recall to compare recall of presentations involving once-presented items with recall of presentations involving twice-presented items. In keeping with the cue overload principle, presentations involving twice-presented items were not recalled as well as presentations involving once-presented items. On the other hand, when we looked at item recall without regard to attribute recall, we found that recall of a twice-presented item was more probable than recall of at least one of two randomly selected once-presented items. This means that recall of twice-presented items could not be adequately accounted for by recall of their individual presentations. We were thus able to confirm our introspection that despite the passage of no more than a minute or so since their occurrence, the presentations of at least some of the twice-presented words were recalled as a generic pair. This conclusion runs counter to conventional wisdom and reinforces our suspicion that, some hopeful recent signs notwithstanding (see Gardiner, 1988; Tulving, 1985), mediationism fails to encourage adequate attention to the experiential side of memory.

A final likely characteristic of research conceived from other than a mediationist perspective is a greater openness to pursuit of unexpected findings. To the extent that research is driven by theory, counterintuitive findings be-

[1] Our rejection of mediationism applies only to what James (1890) called secondary memory—that is to say, to memory for events that have dropped from conscious mind and for which recall consists, loosely speaking, of their being brought back to mind. Only for this "memory in the proper sense" does invoking the concept of mediation take us into an unchartable netherworld. The mediation of primary memory, by contrast, is directly experienced.

yond the pale of theory will not be followed up. Rejecting mediationism would in all likelihood loosen the shackles that theories place on research and so leave researchers more amenable to exploring unexpected findings. The discoveries of penicillin, X-rays, and America have apparently failed to alert students of memory to the possibility of serendipitous findings within their own field. Certainly, we lack a tradition of honest, straightforward reporting of empirical curiosities. Our own experience has been that, with rare exception (as in the case of Peynircioglu & Watkins, 1986), editors insist that such findings be given at least a speculative explanation. This means the concoction of yet another theory, the effect of which is not one of illuminating the finding but of blunting its significance. Interpreting a counterintuitive finding by concatenating ad hoc assumptions about hypothetical entities is merely to obscure its importance, to render it no more significant than an expected finding. We in our laboratory believe that the counterintuitive finding is a precious touchstone for the evaluation of future theories: To the extent that we can develop ways of thinking that make sense of such a finding in a principled way, we will have made a genuine advance in our understanding.

Summary and Conclusions

Research on memory is proceeding at an unprecedented rate, but it is not generating a corresponding rise in understanding. If the philosophers of earlier times could be brought back, they would surely think that understanding had deepened in each of a long list of fields, yet I doubt the list would include the field of memory.

The thesis I have been propounding here is that our understanding is being impeded by mediationism, the doctrine that whatever from the past can be recalled or can otherwise affect thought or action must be represented in a memory trace of some sort. My claim is that, although entirely appropriate for students of the physical substrate of memory, this doctrine is not appropriate for students of memory per se. Students of memory overlook the fact that, for them, the memory trace is merely a metaphor, and in doing so they confuse psychology with physiology. Such confusion has a long tradition and is all but universal today. Worse yet, the current movement to integrate cognitive psychology with neuroscience and artificial intelligence bodes an even more entrenched confusion in the foreseeable future.

Were the memory trace to be recognized for the metaphor that it is, the quest to know the nature of memory for some prior event when that event is out of conscious mind would be abandoned as meaningless. Greater attention would be given to the actual experience of remembering and to the context in which it occurs. Memory would be conceptualized as a biological function that facilitates interaction with the outer world. No longer would the control that the outer world has on memory be reassigned to the rememberer; no longer would memory theorists treat people as though they were utterly impractical visionaries out of contact with their environment (see Watkins, 1989). A major facet of this reorientation would be a concerted effort to disentangle the roles of the rememberer and the rememberer's environment as determinants of the remembering process. Indeed, the subject/stimulus issue could well be as important to postmediationist thinking about memory as the nature/nurture issue has been to thinking about intelligence. Doubtless the issue would no more submit to a straightforward resolution than has the nature/nurture issue, but we are not going to get very far until we have worked it through.

What is holding up these developments? What, in other words, are the factors that sustain mediationism? Three obvious factors are the beliefs that mediationism: (a) is conducive to theorizing at a level of complexity that does justice to the complexity of the

human mind; (b) provides a rich source of research hypotheses; and (c) allows a mechanistic mode of explanation.

There can be no doubt that mediationism is conducive to complex theorizing. Indeed, I doubt that students of memory are any less proficient in bemusing one another than are students of other fields or that our philosopher guests from the past would find our theories of memory especially easy to understand. I might add, for what it is worth, that the assertion by more than one editor that our direct-memory theorizing "insults the intelligence of the reader" is, in a backhanded way, further testament to the complexity of mediationist theorizing. But is complexity of theorizing the best route to a genuine understanding?

The very complexity of the mind is sometimes considered sufficient reason for complexity of mental theories. I believe this to be a bad mistake. Consider the planets, and the problem of trying to make sense of their endlessly changing configurations as they course the night sky. Over the centuries, there have been proposed theories of extraordinary complexity, theories of crystalline spheres and attendant angels, of eccentrics and epicycles. But a successful, widely accepted conception came only with the radically simpler proposals of Copernicus and Kepler. Scientists of whatever field should heed Occam's razor and concede complexity only with the utmost reluctance. The extraordinary enthusiasm with which memory theorists turn this tenet on its head might prompt the charge that they have more interest in demonstrating their intellectual agility than in promoting understanding. However this may be, theories should not be so complex that they cannot be evaluated. The scholastic debate over how many angels can dance on the head of a pin betrays a wonderful conception of the world, but one that went beyond the protagonists' abilities to devise an agreed-upon method for settling the issue. As a result, their deliberations did nothing to further science.

That we need the richness of mediationism to generate research ideas is, in my view, another common misconception. Researchers greatly exaggerate the role their own particular theories play in the formulation of their research. If this role were critical, a description of the method and findings of an empirical study would be insufficient for those not privy to the researcher's theory to judge the merits of the study, whereas surely the fact is that most researchers familiar with the topic would not hesitate to make such a judgment. Such self-assurance shows, at the very least, that a finding can be recast in terms of theories other than the one from which it allegedly sprang. It follows that the real interest value of a research finding is at a level that transcends specific theories.

This reasoning can be extended to call into question the role played by the entire mediationist framework in formulating core research ideas. That we in our laboratory find interest in at least some of the contemporary memory literature is clearly consistent with the notion of a "deep structure" of hypothesis generation, one that goes beyond mediationism. Also, it is logically necessary to go beyond a researcher's theory to account for why some questions are formulated or favored while others are not, even though all may bear on the theory. I believe we underestimate the role that hunches play in the generation of research questions and the extent to which researchers rely on their intuition or tacit knowledge (see Polanyi, 1966). Wittingly or unwittingly, memory researchers capitalize on the unlimited flexibility of mediationism and cover up so disreputable an origin by rationalizing in the language of their respective theories.

The appeal that the mediation metaphor holds for psychologists doubtless owes much to the appeal of mechanism as an explanatory mode: The memory trace bridges the temporal gap between an event and its recall and thereby provides an accounting of memory in mechanistic terms, without recourse to

the concept of action at a distance. In fact, starting with the mediation metaphor, researchers can build any number of mechanistic theories to interpret a given memory phenomenon, and each would have an excellent chance of accumulating a perfect win-loss record. But therein lies the problem: Mechanistic theories are neither compelled nor constrained by the data. Discarding mediationism would resolve the problem, for it would necessitate a different form of explanation. Our own preference is for the discovery and exploration of empirical laws. Such laws would help promote understanding by bringing order to chaos and to that extent would constitute a form of explanation.

In short, mediationism is an illusory basis for theories of episodic memory, or indeed of memory and cognition in general. Moreover, it is insular and an impediment to real understanding. It is time to try something different.

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